the cell parts and their organization were determined by electron microscopy. Contrary to Sjöstrand, Palade did not think either approach could yet yield descriptions of cell parts at the molecular level.[20] Once it was established that the processes of aerobic respiration as a whole were localized in mitochondrial fractions, the natural extension of the research was to seek to localize different enzymes in different components of the mitochondrion. The obvious way to proceed was to apply fractionation again, decomposing the whole mitochondrial system into subfractions that could carry out some but not all the operations of oxidative phosphorylation. If this were successful, researchers could hope to localize these reactions in turn in parts of the mitochondrion that appeared in different fractions. Again Green and Lehninger were the leaders in deploying this strategy, joined subsequently by Efraim Racker.

Taking advantage of the availability of beef hearts from the nearby slaughterhouses in Wisconsin, Green developed a procedure for large-scale fractionation of mitochondria. The procedures he employed routinely damaged mitochondria, but had the advantage of yielding components with different behavior. First, he obtained a *light fraction* that lacked the capacity to synthesize ATP when oxidizing succinate and a *heavy fraction* that retained that capacity. Both fractions, though, phosphorylated ATP when other citric acid cycle substrates were supplied. Green further divided the light fraction (after treating it with 15% alcohol) into subfractions, one of which carried out electron transport but not oxidative phosphorylation (he referred to these as

[20] Rasmussen maintained that a fundamental difference between Palade and Sjöstrand involved their respective relations to biochemistry: "In the science Sjöstrand was trying to build, the electron microscope presumed a certain authority over the territory of biochemists, who were heavily invested in their 'slick' new ultracentrifuges but were in a very weak position to establish for themselves that the cell components they were isolating had not been drastically altered by cell homogenation and the lengthy centrifugation protocols. On the other hand, the Rockefeller way posits a partnership with the fractionation biochemist, and a set of more modest goals for the electron microscopist that prevents conflict with the biochemist partner: mere description of topology of and associations among components in the unfractionated, in situ cell. The Rockefeller cell biologists had a metier whose definition did not entail conflict with the established biochemistry departments at institutions where electron microscopists were finding work in the later 1950s and 1960s" (1997, p. 148). Rasmussen developed this argument as part of a sociological explanation for the greater success of the Rockefeller approach and acceptance of the Rockefeller results. As discussed in the previous chapter, the technique of fractionation itself originated with the efforts of Bensley and especially Claude to link cell structures to biochemical operations. The Rockefeller researchers, including Palade, continued to utilize fractionation as a primary tool in their own research (see the discussion in Part 2 of this chapter of Palade and Siekevitz's collaboration on the endoplasmic reticulum). Thus, another way of viewing the Rockefeller approach is to trace its development as an internally motivated program that, as a happy side effect, minimized territorial conflicts. This differs from Rasmussen's perspective in that it subordinates sociological factors to scientific ones.